

full-pipe flow. It is known that a mixture of water and small bubbles can have a speed of sound which is significantly less than that of the pure liquid without bubbles.<sup>2</sup> This reduction in sound speed is appreciable even for very small values of bubble volume per unit volume of mixture. If this situation obtains in the present context could not this fact be accounted for in the definition of the quantities  $a$  and  $\Phi$  of equations (3) and (4) of the paper? Presumably, a modified scheme of calculation would be required to exploit this slightly more general formulation. Alternatively, were any experiments run using deaerated water? Were any air content measurements made for the water used in the experiments?

## Identification of Potential Failure Nuclei in Rolling Contact Fatigue<sup>1</sup>

**R. L. WIDNER<sup>2</sup> AND W. E. LITTMANN.<sup>3</sup>** We congratulate the authors for their outstanding contribution to the understanding of surface initiated contact fatigue in rolling element bearings. Their excellent metallographic work showing the origin and propagation of surface initiated contact fatigue damage presents a picture that is similar to our observations<sup>4</sup> of surface associated modes of contact fatigue. Since their studies were conducted on through hardened steels in ball bearings and ours largely on carburized steels in tapered roller bearings, a general concept of surface origin contact fatigue begins to emerge. There are, however, some differences between their observations and ours, perhaps because of differences in material composition and microstructure, contact geometry and finish, or the level of contact stress at which fatigue occurs.

While we agree that the loss of EHD film thickness by leakage is responsible for localized wear or glazing around furrows, dents, and other surface flaws we believe that there are also mechanical stress concentrations contributing to this wear. It has been shown that a locally increased contact stress distribution exists at the end of roller raceway contact<sup>5,6</sup> strongly influenced by the transverse radius of curvature at the ends of contact. Therefore, a discontinuity, such as a furrow or dent, has a mechanical stress concentration effect in addition to that resulting from local loss of EHD film pressure.

For dents from handling or debris, there is often a local related asperity resulting from displacement of material when the dent was formed. Together with the end of contact and EHD film effects the higher contact stress due to the asperity often serves as a nucleus for fatigue damage.

Have the authors measured the height of asperities associated with debris dents? What are the relative contributions of the EHD film leakage, end of contact, and asperity effects to the local increase in contact stress that gives rise to surface origin fatigue damage?

<sup>2</sup> See for example, Parkin, Blaine R., Gilmore, Forrest R., and Brode, L., "Shock Waves in Bubbly Water," the RAND Corporation, Santa Monica, Calif., RM-2795 PR (abridged), Oct. 1961.

<sup>1</sup> By J. A. Martin and A. D. Eberhardt, published in the December, 1967, issue of the JOURNAL OF BASIC ENGINEERING, TRANS. ASME, Series D, Vol. 89, No. 4, pp. 932-942.

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<sup>4</sup> Littmann, W. E., and Widner, R. L., "Propagation Contact Fatigue From Surface and Subsurface Origins," JOURNAL OF BASIC ENGINEERING, TRANS. ASME, Series D, Vol. 86, No. 3, Sept. 1966, pp. 624-636.

<sup>5</sup> Moyer, C. A., and Neifert, H. R., "A First Order Solution for the Stress Concentration Present at the End of Roller Contact," *ASLE Transactions*, Vol. 6, 1963, pp. 324-335.

<sup>6</sup> Lundberg, G., "Elastic Contact Between Two Semi-Infinite Bodies," *Forschung auf dem Gebiete des Ingenieurwesens*, 1939, pp. 201-211.

## On Real Fluid Flow Over Yawed Circular Cylinders<sup>1</sup>

**C. DALTON.<sup>2</sup>** Chiu and Lienhard have used the Blasius-series approach toward computing the laminar boundary layer around the yawed cylinder. The calculation of the crosswise boundary-layer velocity is exactly the same as if the flow were completely two-dimensional. The approach was discussed by Sears [8] and was found to be representative of this type of flow. The method used to compute the crosswise velocity is well known to give an erroneous velocity profile past  $\theta$  equal to approximately 70 deg from the leading edge of the cylinder. The computed angle of separation was found to be 108.8 deg which is consistent with the results in the discussion of the Blasius-method by Schlichting [10].

The spanwise velocity field was computed from the Blasius-type crossflow and normal velocity components. The spanwise velocity was not found to separate as far as 120 deg from the leading edge.

Since the crosswise flow was determined to separate before the spanwise flow, the authors conclude that the laminar boundary-layer separation is controlled by the crosswise flow. This statement is probably correct, but the conclusion cannot be drawn on the basis of the calculations performed by the authors.

The actual boundary layer is known to separate at approximately 80 deg from the leading edge for an unyawed cylinder. The large difference between the actual and computed crossflow invalidates any use of the computed crossflow toward the determination of any other boundary-layer property. Since the spanwise flow was determined through the use of this inaccurate representation of the crosswise flow, it is felt that the spanwise flow is at least as inaccurate as the crosswise flow in the region between  $\theta$  equal to 70 deg and  $\theta$  equal to 108.8 deg.

The inaccuracy involved in the determination of the crosswise and spanwise flows severely limits the use of these velocity components as a basis for drawing any conclusions. Therefore, based on their calculations, it is felt that the authors do not have a basis for stating that the crosswise flow controls separation although this is probably a correct interpretation, as indicated by the experimental results.

**A. ROSKO.<sup>3</sup>** The authors state that it is necessary to "learn whether or not the crosswise boundary-layer component is the same as in yawed flow." It seems to me that one can immediately say it is, on the basis of the general independence principle for crossflow over an infinite yawed cylinder; hence one can immediately state that the "separation point" will be at 108.8 deg, as in the corresponding two-dimensional calculations, and it is not necessary to prove this all over again. On the other hand, all this is beside the point, since a classical boundary-layer calculation is not relevant to the problem of separation from a circular cylinder, which is at this time still very much a research problem. To illustrate this point another way, consider vortex shedding from a cylinder of, say, triangular cross-section, where the separation points are known, a priori; namely, at the edges, for both two-dimensional and yawed cases. But this hardly is a proof that the cosine law will hold for the shedding frequency.

In short, the authors appear to have assumed the result rather than proved it. Their assumption is spelled out in the last

<sup>1</sup> By W. S. Chiu and J. H. Lienhard, published in the JOURNAL OF BASIC ENGINEERING, TRANS. ASME, Series D, Vol. 89, No. 4, pp. 851-857.

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sentence on p. 4; it may very well be a valid one, but the discussion preceding it is irrelevant to it.

**L. TREFETHEN.**<sup>4</sup> Would the authors be willing to comment on whether the vortices are parallel to the yawed cylinder, or normal to the flow, or perhaps at an in-between angle? Also, if the cylinder is free to move, would its vibration affect the angle?

## Bubble Trajectories and Equilibrium Levels in Vibrated Liquid Columns<sup>1</sup>

**FRANKLIN T. DODGE.**<sup>2</sup> The authors have derived a partly nonlinear theory to describe bubble motions in vertically vibrated tanks. In Bleich's original analysis (Reference [1] of the paper), as well as in the other extensions to this theory mentioned by the authors (references [2, 3, 4, 5, 6, and 7]), only those nonlinear terms in the equations were retained that were absolutely necessary to show that stationary bubbles existed; even so, the comparison of theory and experiment was reasonably close. The authors, however, have attempted to improve on this theory by retaining certain other nonlinear terms, although various other nonlinearities were discarded. Some other important effects, such as the influence of the tank walls (i.e., finite volumes of liquid), were not considered. Nevertheless, their analysis does indicate that nonlinear effects do have a noticeable role in the bubble behavior since their theory compares slightly better to experimental results than do the previous, more highly linearized theories.

It is not clear, however, what justification there is for retaining the particular nonlinear terms retained in the theory while neglecting others. Perhaps the authors have order-of-magnitude arguments to justify those terms they have retained.

It is worth noting that in a recent study at SwRI<sup>3</sup> which completed our work reported in reference [6] of the paper, the influence of finite bubble size was determined, both by experiment and by a partially linearized analysis similar to Bleich's. It was found that finite-size effects became apparent for bubbles whose diameters were 5 percent or more of the tank diameter. Presumably, the bubbles used in the experiments reported by the authors were smaller than this.

**R. J. SCHOENHALS.**<sup>4</sup> The authors are to be congratulated on their fine contribution in an area of considerable importance, both scientifically and technically. There are two items which are deserving of further comment in the opinion of this discussor. First, it seems that the hydrodynamic force has been formulated for a spherical bubble which is simultaneously accelerating and undergoing a change in volume, while the cited reference gives the resultant hydrodynamic force only for the case of a sphere of constant volume. Could the authors show further detail, or

possibly indicate an additional reference, which would illustrate the principles on which their expression is based? Second,  $\lambda$  is apparently one of the independent dimensionless parameters, among several listed below equation (12), which has an influence on the resulting bubble motion as described by  $Z$ . The experimental measurements of equilibrium levels also show that a variation in  $\lambda$  does have some influence on the observed results. However, the integration method used for analytical prediction of the equilibrium levels yields values which do not depend on  $\lambda$ , as indicated by equation (16) for example. Is this due to the fact that  $\epsilon$  has been assumed to be small in the derivation of equation (16)? Further comment by authors on this second point would also be appreciated.

## Authors' Closure

**R. J. SCHOENHALS**

Additional detail on the formulation of the hydrodynamic force may be found in reference [1] of the paper.

With regard to the vibrational amplitude parameter,  $\lambda$ , equation (16) simply indicates that the motion of a bubble, undergoing *isothermal* pulsations, about an equilibrium level is independent of  $\lambda$ . This is not true for other thermodynamic behavior of the bubble.

**Franklin T. Dodge**

In this paper, the authors attempted to develop equations which describe the bubble trajectory as well as the location of equilibrium levels. In the development, it was necessary to discard certain nonlinearities in order to obtain a solution. As shown in the paper, the assumption of small  $\Delta/A$  is not only incorrect but unnecessary as well. Luckily, this assumption has little effect on the location of equilibrium levels; however, the bubble trajectory and its motion about an equilibrium level are significantly affected by such an assumption. Admittedly, at present, the bubble trajectory is of academic interest only.

Bubble size was not rigidly controlled in the experimental work reported in the paper. An attempt was made, however, to use only "small" bubbles. Perhaps the "finite-size" effect mentioned accounts for part of the discrepancy between the data and theory.

## Effects of Gravity and Surface Tension Upon Liquid Jets Leaving Poiseuille Tubes<sup>1</sup>

**C. P. HUANG.**<sup>2</sup> The neatness of the author's attack on the linearization of two dimensional boundary layer equations is very valuable. This paper contributes to the understanding of the development of velocity profile, exit contraction and gravitational effect of free jets; these aspects of flow behavior are crucial to certain applications.<sup>3</sup> In appraising the results, there are two interesting points for discussion.

<sup>1</sup> By J. H. Lienhard, published in the June issue of the *JOURNAL OF BASIC ENGINEERING*, pp. 262-268.

<sup>2</sup> Senior Mechanical Engineer, Engineering Research Department, Minnesota Mining and Manufacturing Company, St. Paul, Minn. Assoc. Mem. ASME.

<sup>3</sup> For example, Hansen, R. S., Purchase, M. E., Wallace, T. C., and Woody, R. W., "Extension of the Vibrating Jet Method for Surface Tension Measurement to Jets of Nonuniform Velocity Profiles," *Journal of Physics Chemistry*, Vol. 62, Feb. 1958, pp. 210-214.

<sup>4</sup> Tufts University, College of Engineering, Medford, Mass.

<sup>1</sup> By J. M. Foster, et al., published in the March, 1968, issue of the *JOURNAL OF BASIC ENGINEERING*, pp. 125-132.

<sup>2</sup> Senior Research Engineer, Department of Mechanical Sciences, Southwest Research Institute, San Antonio, Texas. Mem. ASME.

<sup>3</sup> Kana, D. D., and Chu, W.-H., "Bubble Dynamics in Vibrated Liquids Under Normal and Simulated Low Gravity Environments," Tech. Rept. No. 8, Contract NAS8-11045, Southwest Research Institute, San Antonio, Texas, Feb. 1967.

<sup>4</sup> Associate Professor, School of Mechanical Engineering, Purdue University, Lafayette, Ind. Assoc. Mem. ASME.